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Charles Thomas Jackson and the First Geological Survey of Maine, 1836-1838

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ABSTRACT

Most historians of science agree on the need to avoid retrospective judgments when evaluating the work of historical figures in science. Thus, when assessing and interpreting the work of Maine's first state geologist, Charles T. Jackson, one must be careful to do so in light of what was known in the period in which he worked (1836-1839), and not upon "what we now know." As an example, Jackson's work in Maine was contemporary with a revision of the stratigraphic column by the British geologists Adam Sedgwick and Roderick Murchison. As a result of their work, purely lithologic descriptions of older stratified rock as "transition" would be replaced by the Cambrian, Silurian, and Devonian systems, determined in large part by characteristic fossils. Application of these systems was problematic in the structurally and stratigraphically complex geology of Maine; moreover, the naming of formations and the development of a stratigraphic column was a source of contention in American geology throughout this period.

Similarly, Jackson's diluvial interpretation of the glacial drift that occurs in the state went far toward explaining the sources of soils in Maine. The explanatory power of the diluvial theory permitted Jackson to describe phenomena, such as glacial striations, which might have been otherwise overlooked.

Jackson's interpretation of the geology of Maine has been the subject of criticism by historians of science and geologists alike. A fresh look reveals a field researcher, committed both to his task and to his methodology, grappling with the complex geology of Maine.

INTRODUCTION

Between 1837 and 1839 Charles Thomas Jackson (1805-1880) published three reports of a state-financed geological survey of Maine (Fig. 1). These reports make excellent reading and today remain interesting in their own right, but contradictory judgments about their scientific worth pose a problem for the historian of science. Contemporary assessments of Jackson's surveys in Benjamin Silliman's *American Journal of Science* were laudatory (Silliman, 1839). Jackson was a member of the scientific elite in the United States. This, combined with favorable acceptance of his field work and interpretations, suggests his being a competent, respected geologist. Élie de Beaumont, with whom Jackson studied in the early 1830's, makes kind mention of him in the three-volume *Notice sur les Systèmes des Montagnes* (Woodworth, 1897). The obituaries and memorials published shortly after Jackson's unfortunate "insanity" and death may be read alternatively as apologia or as tributes.

Later judgments have been less enthusiastic. Merrill (1904), in his ambitious study of the state surveys, concluded that "These reports, examined in the light of to-day, contain very little which would be considered of geological importance." Aldrich, in a more recent reassessment (1981), reaffirmed the essence of that judgment, but tempered it by suggesting that Jackson "should be credited with providing the science with basic data about states without which other American geologists would have found their work lacking."

The disparity between the contemporary assessment of Jack son's published survey of Maine and later assessments is intrigu ing. Two possible explanations for the disparity will be explored in this paper. The first of these is that Silliman, in praising Jackson in *American Journal of Science*, for example, made an error in judging Jackson's science, and that this error was corrected by later, less biased and more sophisticated observers. Set

Figure 1. Charles Thomas Jackson.

against this interpretation is the second possibility: that the context in which geology was practiced changed significantly between Jackson's time and the period of reassessment, such that the virtues of Jackson's work became obscured while the vices stood out in sharp relief.

These options may be overdrawn. Overstatement of historical judgments appears, however, to be a systematic flaw in the history of geology which, according to Greene (1985), may be characterized by three approaches to writing and interpreting history: the attack, the celebration, and the review. Correction of these obvious biases in the history of geology has lagged behind the remarkable revision carried out in the histories of other sciences, notably the history of physics, over the past twenty years. Recent investigators, notably Greene (1982) and Rudwick (1985) have begun to cement a more rigorous tradition in the history of geology, but as Turner (1986) has pointed out for the case of Rudwick's "non-retrospective" history of the "Devonian controversy," this leads to a transfer and diminution, not an elimination of bias. If Turner is correct in pointing out that "all historical analysis requires that the historian exercise judgment," what, then, are to be the criteria for judgment? A provocative essay by Donovan (1981) suggests two apparently mutually exclusive approaches based upon differences in interest between geologists and historians. Geologists, on the one hand, have sought a history that

> provides object lessons in scientific method and reinforces the belief that modern geology is a fundamentally empirical science. Such a history exults in conceptual conflict. . . . And, if the founding of modern geology is to be seen as the establishment of a discipline, then this approach defines the discipline in terms of its conceptual structures and its subject matter.

In this tradition, debate between Neptunists and Vulcanists is disposed in favor of the latter through recourse to some physical evidence – in this case, the basalts of France. Kuhn (1970) has explained such an approach to the history of science as pedagogically functional but inadequate for several reasons, one of which is the naive realism that bespeaks of "a fundamentally empirical science." Kuhn's criticism of "whiggish" history of science came at a time when social and intellectual historians turned their backs upon naive realism in favor of an overarching theory of social constructivism. The result has been an "externalist" history of science which may leave all empirical evidence for facts, laws, and theories out of focus in the overall depth of field, preferring to envision debates, such as those of the Neptunists and the Vulcanists, the Catastrophists and the Uniformitarianists, as exercises, as Donovan puts it, of "social control." The result of this second tradition, as several critics have suggested, may be a history of science without science (Donovan, 1981; Greene, 1985).

As Donovan has pointed out, neither of these traditions is wholly adequate to characterize the history of a science; both are amplification/reduction devices which pay too high a price for their overall resolving power. Very recently, Greene (1982) and Rudwick (1985) have approached the origins of modern geology in ways that, while differing one from the other in such aspects as narrative technique, nevertheless manage to preserve science while doing justice to the cultural milieu in which science must take place. In his conclusion to *The Great Devonian Controversy*, Rudwick suggests that

> it is possible to see the cumulative empirical evidence in the Devonian debate, *neither* as having determined the result of the research in any unambiguous way, as naive realists might claim, nor as having been virtually irrelevant to the result of the social contest on the agonistic field, as constructivists might maintain. It can be seen instead as having had a *differentiating* effect on the course and outcome of the debate, constraining the social construction into being a limited, but reliable and indefinitely improvable, representation of reality. (Rudwick, 1985, p. 455-456)

Geological interpretation, in other words, may be *underdetermined* by empirical evidence, but it is not *undetermined* by it. Put another way, the rocks are not self-interpreting, but they do *constrain* interpretation in a way that the subject matter of other sciences sometimes does not. With this position as a guiding bias, it should be possible to satisfy the needs of both the geologist and of the professional historian when reconsidering a figure such as C. T. Jackson. The careful historian may attempt to discern the difference, as enhanced by the obvious advantage of hindsight, between errors of applying a chosen methodology and "errors" of methodological choice. In this paper, the scientific context in which Jackson's geological education and survey was carried out provides a background against which to assess Jackson's reports. With the context made partially clear, several of the problems faced by Jackson will be examined in greater detail. Before concluding and reassessing the value of Jackson's work in Maine, several aspects of his later life are considered as possible "circumstantial" but supporting evidence for the devaluation of Jackson's science by Merrill and others. Although a closer examination of the social and political contexts of the survey might well prove valuable, they are beyond the scope of this paper and remain areas for fruitful study.

THE CONTEXT

After 1800 and through the present day, all periods in geology are both significant and transitional. To isolate any one period and attempt to describe it using these qualifiers would be to say very little. Nevertheless, the years 1830 to 1840 have a special significance for the geology of Maine, for it is in this decade that the stratigraphy of lower Paleozoic rocks coalesced in the work of Adam Sedgwick and Roderick Murchison. Also, Louis Agassiz developed and published his glacial theory. In later years, these developments were applied successfully to the interpretation of Maine geology, but they are virtually absent in the work of C. T. Jackson. The modern reader of Jackson's reports may, accordingly, regard Jackson as having hailed from the rearguard of science and as an unfortunate choice for Maine's first state geologist. The historical record shows, however, that this was not the case. Quite to the contrary, Jackson was at the very core of the social and intellectual center of American geology from 1835 to 1845 (Rudwick, 1985, p. 420-421). An analysis of Jackson's competence and influence during the decade of the Maine, Rhode Island, and New Hampshire surveys shows him to have been among the elite of American geologists. Jackson was among Governor Marcy's first choices for head geologist of the New York survey, but apparently was passed over because Marcy felt he could not afford Jackson's services (Reingold, 1979). Jackson published in the *American Journal of Science* and scientific journals in the United States and in Europe; he helped found the Association of American Geologists and Naturalists (later the American Association for the Advancement of Science), and served as chairman of that organization for the year 1845-1846 (Woodworth, 1897, p. 86). It is only later that Jackson's status was reduced to a lesser position among American geologists.

C. T. Jackson was born in Plymouth, Massachusetts, on July 21, 1805, the son of Charles Jackson, a merchant, and Lucy Cotton Jackson. Demographically, such beginnings suited Jackson to a career in science. Better than one third of American scientists active from 1800-1863 were New Englanders; a third came from families with commercial backgrounds; and many were trained in medicine, although to have earned a doctorate and to have gained post-doctorate training, as Jackson did, was unusual (Elliott, 1982). There seems to be no evidence that Jackson had an early interest in geology. Instead, his tutors, James Jackson and Walter Channing, prepared him for the study of medicine (Woodworth, 1897, p. 70). With that preparation, Jackson continued his training at Harvard College under John White Webster.

Webster, Erving Professor of Chemistry and Mineralogy during Jackson's tenure as a medical student, was a popular lecturer at Harvard and enjoyed a long teaching career there before being hanged for a sensational murder. He kept abreast of English and Continental journals and spent enormous sums of money on chemical apparatus and mineral samples (Cohen, 1950). Webster also had a keen interest in geology. In 1826 he published "a somewhat detailed account of the geology of Boston and vicinity" and worked on the Roxbury conglomerate, but deferred interpretation of that formation because he found it "inexplicable with the geological information then available" (Merrill, 1904). From Webster, Jackson learned techniques of chemical analysis which he later employed in his state surveys.

Although Jackson seemingly could not help but be influenced by such an individual as John White Webster, Woodworth suggests that Jackson's first interest in mineralogy "was aroused, while staying in Lancaster, Mass., by finding the crystals of macle or chiastolite which there abound in the glacial drift." During the summer of 1827, Jackson traveled to Nova Scotia with Francis Alger where they collected minerals; he returned there for a second field trip with Alger following his graduation from Harvard in 1829. These collecting trips formed the basis of Jackson's first published paper (coauthored by Alger), "A description of the mineralogy and geology of a portion of Nova Scotia" in Silliman's *American Journal of Science* (Jackson and Alger, 1828 & 1829). He also visited New Jersey and New York with Gerard Troost, later the state geologist of Tennessee, and William Maclure, whose Wernerian classification for American rocks Jackson would still be using, with modifications, nearly twenty years later (Woodworth, 1897, p. 71).

Jackson graduated from Harvard in 1829 and traveled to France in the fall of that year, "evidently with the intention of fitting himself for a high place in the profession for which his tutors had prepared him" (Woodworth, 1897, p. 70). In Paris he studied at the University of Paris and attended lectures at the École de Médicine, the Collège de France, and the École des Mines. There he met Jean-Baptiste Élie de Beaumont, with whom he "formed a friendship which lasted many years" (Woodworth, 1897, p. 71). Élie de Beaumont would later write of Jackson, somewhat noncommitally, that he was "bien connu par ses travaux" (Woodworth, 1897).

As it frequently was throughout his life, the timing of Jackson's study in France was unfortunate. Through this period, Élie de Beaumont was primarily concerned with mapping the upper Paleozoic in France (Rudwick, 1985, p. 91); only later would he concern himself with the structure of the Alps and what was known then as "dynamical" geology, a subject that, had he lectured on it, might have been more useful to Jackson in the northern Appalachians (Greene, 1982). Nevertheless, Jackson traveled extensively throughout France and Italy, and saw a good many rocks. Also, for once, his timing improved: he witnessed the 1831 eruption of Vesuvius. But his time in France would have significant consequences later, for it distanced him from the growing English stratigraphic tradition. This was, in part, a matter of choice and of nationalistic prejudice. England was still an enemy nation for Jackson, and it is difficult to determine how much his patriotism affected his scientific judgment.

Jackson's work in Maine occurred at the same time as the consolidation of the Devonian system by Roderick Murchison and Adam Sedgwick in England. This consolidation was the background for a major scientific debate over which characteristics of a stratum should be used to date it. In his study of that debate, Rudwick outlines some of the points of contention. For many geologists, including the English field geologist Henry de la Beche and the father of the Oxford school of geology, William Buckland, fossils were but one of several indicators of age – just as important to consider were lithology and position in the sequence. Figure 2 shows the sequence used and advocated by Buckland and de la Beche. Murchison and Sedgwick, conversely, advocated a sequence determined primarily by paleontological evidence, following the lead of the English engineer, William Smith.

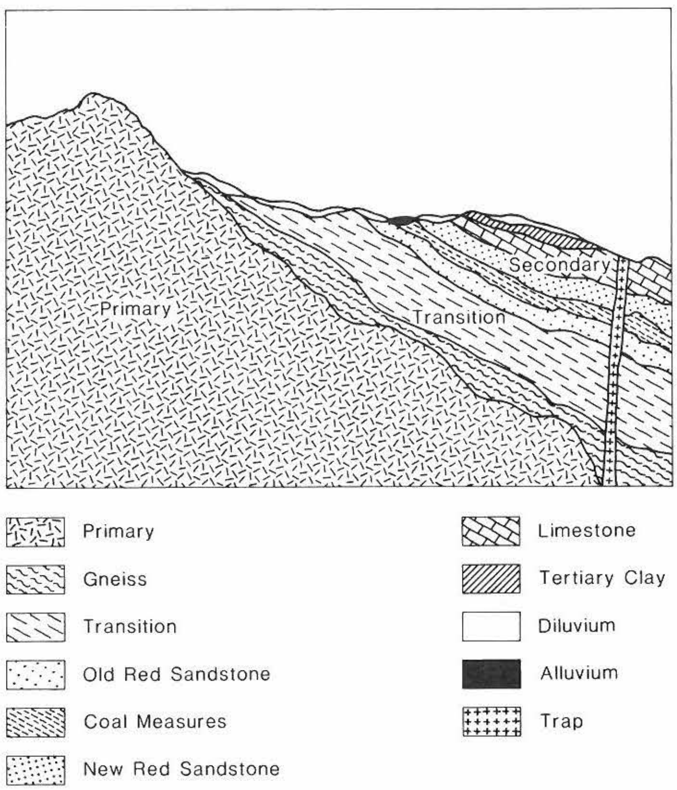

Figure 2. Ideal section as advocated by C. T. Jackson, showing the positions of the Old Red and New Red Sandstones.

The Devonian controversy began as the result of an error in structural correlation by de la Beche, who placed rocks containing fossil land plants at the bottom of a sequence in Devonshire overlain by Murchison's Silurian system. Murchison found this quite impossible; there were no land plants in the Silurian. The battle over the Devonshire fossils raged over a full decade, and was finally resolved by inserting the Devonshire fossils between the Silurian and the Coal Measures, or Carboniferous. Moreover, Louis Agassiz showed that the Old Red Sandstone (see Fig. 2) correlated with the Devonian system. Ultimately, these conventions were applied to the bedrock geology of Maine, but not by Jackson. He continued to reject the English conventions even after 1840, and his rejection of "Cambrian and Silurian as names for our rocks" (Jackson, 1840) did not enhance his stature in the eyes of later historians of geology.

Upon his return to Boston, Jackson set up a chemical laboratory attached to his house at 21 Green Street, married Susan Bridge of Charlestown in 1834, and began to establish a reputation as a physician and chemist. Between 1829 and 1837 he published his paper with Francis Alger on the geology of Nova Scotia; an account of the chiastolite that had inspired his interest in mineralogy; a paper on the conglomerates and dikes of Roxbury; and several chemical analyses of coal, water, and copper. In the commercial directory of Boston, Jackson listed himself as a physician, "but finding his services in demand as a chemist and mineralogist, he gradually and not against his inclination, entered upon a career in these pursuits" (Woodworth, 1897, p. 71). By 1835, with his reputation for field work established by his publication on Nova Scotia, Jackson was hired by private concerns in Maine to examine several possible commercial prospects near Thomaston, Foxcroft, and Williamsburg. When the legislatures of Maine and Massachusetts set aside funds for a survey of the state and its public lands, Jackson was a sensible if not inevitable choice for the position of state geologist.

Although a full discussion of the economic, political, and scientific significance of the early state surveys is beyond the scope of this paper, a glance at the constraints placed upon the early geologists and their surveys will help to round out the context. Hendrickson (1961) has argued that the first state surveys were authorized primarily for economic reasons. Aldrich (1979), however, has shown that the geologists were also interested in contributing to the growth of geologic knowledge, independent of economic concerns. The debates in England and on the continent about stratigraphic conventions were echoed by the state geologists of the 1830's as they searched for uniform meanings for terms such as "formation," "group," and "series" (Aldrich, 1979). And Schneer (1981) has cautioned against searching for systems in the work of the state geologists of this period. The "microgeologic techniques" of Abraham Gottlob Werner, the German geologist whose classification of lithology provided a foundation for the study of rocks, provided necessary tools for field geology in the vast, unmapped, and undescribed wilderness of the North American continent. It was to these microgeologic techniques that the state geologists turned, for the most part; theory and system could come later. Rudwick (1985) rounds out the context when he captures a sense that geology in the 1830's was a "new, exciting, and fashionable science." From the scientific societies of London, Philadelphia, and Boston to the town of Blue Hill, where "no less than forty" townspeople joined Jackson in his ascent of Blue Hill to measure its altitude (Jackson, 1838, p. 38), new ideas about the earth and its history combined with what Rudwick calls "the romance of fieldwork" to make geology much more than a merely economic activity.

THE PROBLEMS OF THE GEOLOGY OF MAINE

C. T. Jackson spent three full field seasons in Maine, from 1836 to 1838, and published three annual reports based upon that field work. In each of the reports, Jackson introduced the year's work with comments about the nature and value of geology, followed closely by an account in "travelogue style" of apparently "undigested field notes" (Aldrich, 1981, p. 6) in which he described the lithology of each place visited earlier in the year. Here, an attempt is made to recapture the essence of the field work and reports by examining some of the problems Jackson attempted to solve.

In late June of 1836, C. T. Jackson was formally contacted by an emissary from the state of Maine to prepare a geologic survey of the state. Because public lands in Maine continued to be held by Massachusetts, that state contributed to the funding of the survey. The joint commission was received by Jackson early in July. Given the short field season in Maine, he was off to a late start. Only 31 years old, Jackson must have felt daunted by the prospect of surveying an area as large as Maine. He wrote that he "hesitated at first, doubtful whether I should be able to accomplish so Herculean a task and do justice to the subject" (Jackson, 1837, p. 9). Several considerations helped to settle the matter of planning. First, he had been asked by Robert P. Dunlap, the President of the Board of Internal Improvements, to begin his work at the mouth of the St. Croix River, where previous field work led Jackson to suspect a deposit of coal associated with the red sandstone. Second, the survey would take advantage of exposures along the coast. Beyond this starting point, the survey would proceed along a division of Maine into "squares" bounded by the St. Croix and St. John, Penobscot, Kennebec, and Androscoggin Rivers. This division had the advantage of organizing the state into roughly equal areas, and it also took advantage of the rivers for transportation and as likely prospects for outcrops (Jackson, 1837, p. 11). In the first field season, Jackson was assisted by Dr. T. Purrington of Brunswick for the state of Maine, James T. Hodge for Massachusetts, and F. Graeter (draftsman). In the second season, the draftsman was eliminated as an economy, while Mr. W. C. Larrabee replaced Dr. Purrington for Maine. In the third season Jackson was assisted by Dr. S. L. Stephenson – whose report on the headwaters of the Androscoggin River is appended to the third report – and Ariel Wall of Holloway. Figure 3 follows the progress of Jackson's field work through the 1836, 1837, and 1838 field seasons. Altogether, the survey cost the state $12,000 (Merrill, 1920).

Toward the end of his first report (Jackson, 1837, p. 86), Jackson made this statement about his philosophy of science:

> I feel I am attempting to compress the geological history of a great country into too narrow limits. . . . I only hold the pen; Nature dictates the facts, and I have presumed to put in, here and there, a word of interpretation, which I hope may not come amiss.

This statement has led Aldrich (1981) to conclude that Jackson's ". . . primary mission was to describe and record, not to theorize. . . ." Apart from philosophical considerations about whether such a theoretical/descriptive distinction is possible, one might wonder whether Aldrich's characterization of Jackson's "primary mission" is correct. Did Jackson "only hold the pen" and attempt to apply Francis Bacon's scientific method of theory-free induction? To answer this, it is necessary to examine the geological problems faced by Jackson in Maine, and to comb through them for a sense both of the solutions and of the *kinds* of solutions he proposed for them.

Topographic geology

The first of these problems concerned the topography of Maine. Following the lead of Hitchcock in Massachusetts, Jackson made no clear distinction between measuring and describing topography, on the one hand, and noting lithology on the other (Aldrich, 1981, p. 6); herein, each will be dealt with successively.

In 1836, little topographic control had been established for maps of the state, at any scale. Jackson depended on nautical charts, town maps, and the map of the state published by Greenleaf in 1830. From his first field season, Jackson carried a barometer in order to establish altitudes for the mountains he ascended, and would take the bearings of other points of high relief from each summit; but apart from these attentions and occasional corrections to existing maps (Jackson, 1837, p. 10 and 63), he did no systematic mapping. The modern reader may express surprise that Jackson would attempt any kind of survey in the absence of proper maps, or that he seemed to consider mapping outside the demands of the survey. Two observations may serve to explain this deficiency in the plan for the survey. The first is the obvious size and cost of a mapping project, which may have been economically unjustifiable because Jackson's geological survey was funded by the state on a season-to-season basis with no promise of renewal. Accordingly, Jackson sought to provide the greatest amount of information at the least expense of time and money. Thus, the survey years were a reconnaissance in the strictest sense of the term. Moreover, Jackson found his funds reduced in the second year of the survey and argued that the minimal topographic information he wished to provide was compromised by the cut. The result is an unsystematic account of the topography of the state. These measurements are dispersed throughout the reports as lists of bearings and altitudes complemented by verbal descriptions of regions, and the occasional woodcut in which relief is characteristically exaggerated.

The reconnaissance nature of the survey also had an effect on Jackson's discussions of stratigraphy and his descriptions of lithology, but an explanation of Jackson's lithologies and stratig-

Figure 3. Approximate routes followed by Jackson during the three field seasons of his survey.

raphy should not be reduced to economic grounds. The dominant concern of geologists in the 1830's for the stratigraphic sequence has been discussed above; Jackson's handling of this problem, accordingly, will tell something about his competence as a field geologist.

Stratigraphy

Jackson began his 1836 field season at the mouth of the St. Croix River, a significant area because Jackson suspected that it would represent the western extent of the "New Red Sandstone" and underlying Coal Measures of New Brunswick and Nova Scotia, which Jackson had previously seen at first hand. The importance of this aspect of the geological survey was underscored by the boundary dispute with Great Britain over the north and east boundaries of Maine. If the Passamaquoddy Bay area or regions to the north along a line through Calais and Houlton contained coal deposits, or even if they could be correlated with the Coal Measures, the fact would be significant in any settlement of the dispute. "Here," notes Aldrich (1981, p. 7), "was 'mission-oriented' geology indeed."

In the area surrounding Passamaquoddy Bay, Jackson recorded the occurrence of red sandstone extending along the coast from Perry to Robbinston (Fig. 4). Having found charred fossil plants near Pulpit Rock, which he described as "marine," and noting the composition of the sandstone and contiguous lithologies, Jackson confidently correlated it with the New Red Sandstone, stating that "it is . . . an undoubted fact, that the sandstone in question is identical with the red sandstone of Nova Scotia which contains gypsum, salt springs and coal." He believed the sandstone to be an apparent "continuation of that, which exists in New-Brunswick, and in which the bituminous coal of Grand Lake is probably contained" (Jackson, 1837, p. 17). According to the stratigraphic conventions followed by Jackson, the New Red Sandstone was a top bed marker of coal-bearing strata; thus, "No geological observations would imply, that the red sandstone in question should not contain coal, for if it should be found equivalent to the new red sandstone formation of Europe, it will belong to the upper coal series" (Jackson, 1837, p. 18).

Jackson's correlation of the Passamaquoddy sandstones provides a useful case study for examining his commitment to a stratigraphic methodology. Before exploring the minutiae of the case, it is important to point out that Jackson often reached for the gross correlation: a case in point is his promise, for a "future excursion" to "trace the known coal-bearing strata of New Brunswick, up the St. John, from the Grand Lake coal mines to the Aroostic; and thence, if the strata are found to be continuous, following their course until they intersect the public lands" (Jackson, 1837, p. 69).

Jackson's tendency to reach for a gross correlation does not entirely explain why he placed the sandstones in Perry with the New Red Sandstone. Perhaps he knew nothing of the Old Red Sandstone. Alternatively, he may have discounted or ignored the possibility of its occurrence in America. The former is un-

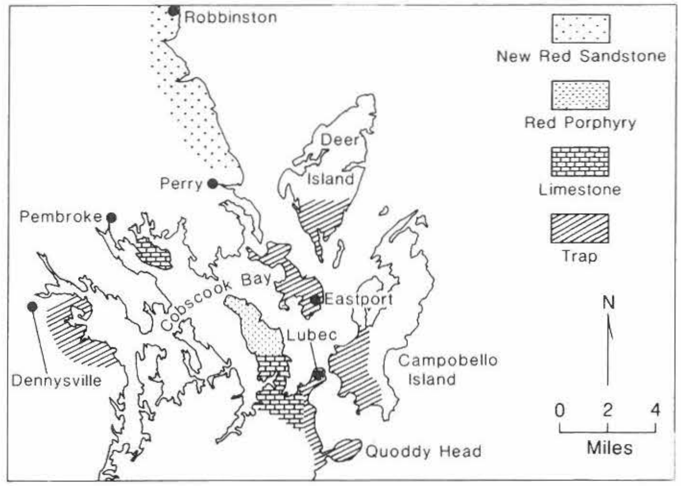

Figure 4. The "New Red Sandstone" of Perry.

likely; although there is no evidence that he was familiar at this time with Henry de la Beche's textbook (1831), and it is improbable that he could have seen Buckland's "Bridgewater Treatise" (1836) at this early date, he certainly had read both by the time of the third report, which contains a reaffirmation of the correlation. Both Buckland and de la Beche showed the Old Red Sandstone below the Coal Measures, and offered Jackson an alternate interpretation. But he seems never to have entertained the alternative. The latter explanation is more likely. The Old Red Sandstone, an important stratum in England and parts of Europe, does not occur among the rocks he saw in Europe, or among those in Nova Scotia where he cut his geological teeth. What should lead him to suspect that the rocks of Passamaquoddy Bay should differ from those of Nova Scotia or New Brunswick?

The answer is to be found in the two criteria for correlation he mentions: fossils and superposition of strata. Inasmuch as the 1836 field season took place at the height of the Devonian debate, it is inconceivable that fossil evidence could have settled the matter, even if Jackson had known the details of the debate and had taken a position in it, which he did not, prior to 1840. The "charred fossil plants" at Perry he identified as *Fuci*; he also found the tests of "Natica (socialis?)" in limestone at Machias. These, Jackson designates as "fossils of the secondary series" (Jackson, 1839, Catalogue, p. xvii). Again, however, the Devonian system was as yet inchoate and could not have been used in 1836 to decide the age of the Perry strata. One is left with superposition and Jackson's promise to trace the New Red Sandstone through New Brunswick to the rocks west of the St. John River. This, however, may have been impossible given the international boundary dispute; a proper traverse from Grand Lake through to the St. John River by a state-commissioned geologist, however innocuous, would have been construed by the British as impolitic at best and as an act of war at worst. And, of course, such a traverse would not have been innocuous, given the "mission-orientation" of the research.

Thus, the "New Red Sandstone" correlation was the best possible interpretation for the Perry strata at the time and given the circumstances. Jackson was cautious, moreover, to point out that "it not unfrequently happens that some members of the coal series are wanting, which may be the case here. It is however worthy of exploration; and by boring through this rock in a few places, the question may be settled at little expense, to those who may enter on the task" (Jackson, 1837, p. 18).

Subsequent work has established that Jackson made an error in his correlation, and that the sandstone of the Perry Formation is the Old Red Sandstone. But it is important to understand that Jackson's error was a *correlation* error, not necessarily an error of stratigraphic methodology, given the shifts of methodology taking place at the time. Indeed, the error may appear methodological only in retrospect.

To understand this, Jackson's stratigraphy must be examined in the context of the 1830's. It is clear from the texts of his reports, from the definitions in the glossary printed with the first report, and from the following comment in the introduction to Jackson's report on the geology of Rhode Island (Jackson, 1840), that Jackson's formations followed the "Wernerian" style:

> A numerical division, will doubtless be found preferable to any of those fanciful names, which have lately been proposed for certain groups of strata of the Transition series, and it is evident that the names Cambrian and Silurian, proposed for certain groups in England, will never be regarded in this country as appropriate terms for our rocks; and I observe that they have not been adopted by De la Beche, in his late Report upon the Geology of Cornwall, Devon, and West Somerset. (Jackson, 1840, p. 11)

This notorious comment, often quoted (Woodworth, 1897, p. 74; Merrill, 1904, p. 347-348), may be interpreted along several lines. Was it a reaction to the parochialism of using regional names to refer to a global sequence? If so, Jackson was in good company, concurring with the Rogers brothers of the Pennsylvania and New Jersey surveys (Aldrich, 1979, p. 136). Or was it a statement of commitment to Wernerian categories? This is the position taken by Merrill, who wrote that Jackson "was conservative almost to the point of obstinacy, as is shown by his steady adherence to the older forms of classification, though finding it necessary to depart somewhat from the ideas of Werner" (Merrill, 1904, p. 290). Woodworth, referring to the preface of the Rhode Island report, suggests that Jackson "was not an advocate of biological methods in geology," and that "his predilection for chemistry and mineralogy manifestly made geology for him a mineralogical rather than a stratigraphical science, and the peculiarly crystalline character of the rocks of New England fostered this view of geology" (Woodworth, 1897, p. 73). While Woodworth's points are well taken, they ignore the possibility that Jackson's "predilection" represented an *evolution* in commitment rather than mere obstinacy, as Merrill would have it. In his first annual report, Jackson discussed "fossils, which are very important, in determining the relative age of the rocks, in which they are found" (Jackson, 1837, p. 27). Wherever he found them in Maine, Jackson commented on the fossils. But the fossils Jackson found in Maine occurred predominantly in the "tertiary" clay formations and in erratic boulders. While the former were useful in establishing the age of the clays, the latter were useless unless the source of the erratics could be traced. Given the status of stratigraphy in the period and the relative absence of fossils in the rocks of Maine, a numerical division of sequence, established through lithology, superposition, and structures, was the more useful – and often the only – correlation tool. Even in 1987 fewer than one fourth of the formations and their members shown on the Bedrock Geologic Map of Maine have paleontological controls; of those, one fifth are not correlated to a single system (Osberg et al., 1985). If Jackson's predilections made geology a mineralogical rather than a biostratigraphic science, a good number of geologists have followed him in that tradition. If this is the case, it may be reasonable to conclude that Jackson made an attempt to forestall a set of conventions which had only tenuous application to vast expanses of rock. Jackson's notorious comment about the Silurian and Cambrian systems is followed in context by his cautious plea that "a new nomenclature would be wholly irrelevant while Geology is in its present imperfect state, and it is highly desirable for us to maintain the old landmarks, until new ones can be established by general agreement" (Jackson, 1840, p. 12).

Bedrock geology

In each of his annual reports, Jackson promised to provide a geological map of Maine as part of a final report. Although the legislatures of Maine and Massachusetts did not require one, Jackson seems to have considered a map a prerequisite of a satisfactory survey. The following remark from the second report contributes to the sense that Jackson hoped to prepare some systematic summary of the data he collected in the state:

> How is a geological survey to be conducted? This question may be answered as follows: The district in question is first to be examined, so as to ascertain the order of strata, and the relative age of each stratum, while, at the same time, the intersecting rocks are to be observed. The method pursued is first to form a plan of operations, so that all the observations may be recorded, in an orderly manner, that no confusion may arise in the completion of the work. (Jackson, 1838, p. ix)

A non-ironic reading of this paragraph requires a large measure of charity; the confusion of the published reports may be explained away by arguing that the cessation of funding precluded "completion of the work." But confusion is the dominant feature of the reports. As noted above, Jackson's stratigraphic methods gave priority to the superposition and composition of the rocks. With his assistants, he

> described all the rocks exactly as we saw them, and the annual reports must be regarded as the mere field notes that may serve for a more thoroughly rational system, illuminated by a comparison of the results with each other. . . . (Jackson, 1839, p. ix)

It is difficult, however, to read any "rational system" into the reports because Jackson provides little sense of scale in his "field notes." Where he describes intercalated limestone and argillaceous slate, the reader usually gets no indication of the relative thicknesses of the beds, and few clues to the dominant lithology. This confusion is systematic, and Jackson's comments about how a survey "is to be conducted" may have been disingenuous. As Aldrich (1979) has found in her study of the state geological surveys, "successful surveys courted the voters partly by discoveries in economic geology. . . ." By his own admission, Jackson's primary interest was to find economic benefit from the geology he described (Jackson, 1839, p. 1-2). Jackson's admitted preference for discussions of fine-scale lithologic features and occurrences of minerals – bog iron deposits, attention to minor beds of limestone, a mention of the use of chlorite by Indians for making pipes – obscures the picture of regional geology. The state legislature, in bringing the survey to a close in 1838, may well have considered the former sufficient, the latter unneeded.

Jackson did, however, provide lithologies aplenty in his reports; from these, a blurred mental map of the bedrock geology of Maine emerges. But if Jackson's reports are "mere field notes," is it possible to construct a true geological map from them? Such a suggestion is tempting, and several clues to the appearance of such a map – Jackson's lithologic descriptions; his tendency toward gross correlation combined with his intuition that the "general direction of strata in Maine is N.E. to S.W." (Jackson, 1837, p. 11-12) and measurements of beds confirming the intuition; the handful of sections that appear in the reports; Jackson's comment that the promised map would be shaded; etc. – make that temptation irresistible. Accordingly, Figure 5 shows a geological map based upon Jackson's field notes. It is important, however, to understand that this is *not* in any sense the map that Jackson himself would have prepared, for a number of reasons.* First, despite his written intentions of preparing a shaded or tinted map, the maps of Rhode Island and New Hampshire (see Aldrich, 1981, p. 7 and 9) show a numerical symbolization of bedrock on a town-by-town basis. Second, few contacts are indicated in the reports; even if they were, they would have been of minimal value in the absence of a properly contoured base map. Also, Jackson had no clear understanding of the significance either of large or small-scale folds, although how this may have affected his decisions about mapping is a matter for pure conjecture. Finally, modern knowledge, based upon the 1985 Bedrock Geologic Map of Maine (Osberg et al., 1985), could hardly be banished from the cartographer's bias. Accordingly, the accompanying map is best read as a diagram or summary of Jackson's lithologies of rock units organized around a presumption of Jackson's presentation of stratigraphy.

*As David Gooding (1986) has recently pointed out, however, the reconstruction of experiments conducted by historical figures in science is fraught with complications. Thus the reconstruction of an experiment – or, in the present study, a geological map – that was never attempted by a former scientist probably should not be attempted by the historian.

Woodworth, in his memorial to Jackson, provided the following insight:

> Dr. Jackson did not always push his theories of geological phenomena to the fullness of conclusion and statement which would enable us at the present day fully to understand them. He had too many irons in the fire to do as he would with all of them. (Woodworth, 1897, p. 83)

Nowhere is this better seen than in the unkept promise of a geological map of Maine.

Diluvialism and Geomorphology

As it must have been for Merrill, working at the turn of the twentieth century, the most striking aspect for the modern reader of Jackson's reports is the recurrence of references to the "mighty rush of waters" (Jackson, 1837, p. 65) that carved "diluvial scratches" in every part of Maine. According to Merrill,

> Jackson's views on the glacial deposits were naturally crude. The "horsebacks" (ridges of glacial gravel) were regarded as diluvial material transported by a mighty current of water. (Merrill, 1904, p. 347)

Apart from a reference to Jackson's "criterion for distinguishing ice-borne from water-transported detritus," Woodworth the apologist never discusses Jackson the diluvialist (Woodworth, 1897, p. 83). With a greater stock of hindsight, Aldrich states that Jackson

> adhered to the theory of a catastrophic deluge, patterned closely on the Biblical flood, to account for these phenomena. (Aldrich, 1981, p. 6)

Although it is true that Jackson was a diluvialist, it is not at all clear that the deluge as a mechanism for transporting the diluvial materials was "patterned closely on the Biblical flood" or that a reading of "catastrophic" in the sense in which Whewell, the nineteenth-century English geologist and philosopher, used the term precisely describes Jackson's views on the matter. There is at least some evidence that Jackson postulated more than one period of global flooding, as when he wrote of "the *last* grand deluge that overwhelmed the globe" (Jackson, 1837, p. 74-75, emphasis added). Such a suggestion of cyclical flooding is not true to the Biblical account; it is rather an echo of one of several theories held by Buckland (Rupke, 1983). Moreover, the words "cataclysm" and "catastrophy" must be approached with care when they appear in the reports of geologists prior to 1840, the year in which Whewell framed the catastrophist-uniformitarianist debate, making these terms to some extent taboos in geology. When these roadblocks to historical inquiry have been cleared away, what emerges is remarkable growth in Jackson's commitment to the diluvial theory in the years 1836-1839, as well as in his ad hoc explanations for anomalies to the theory.

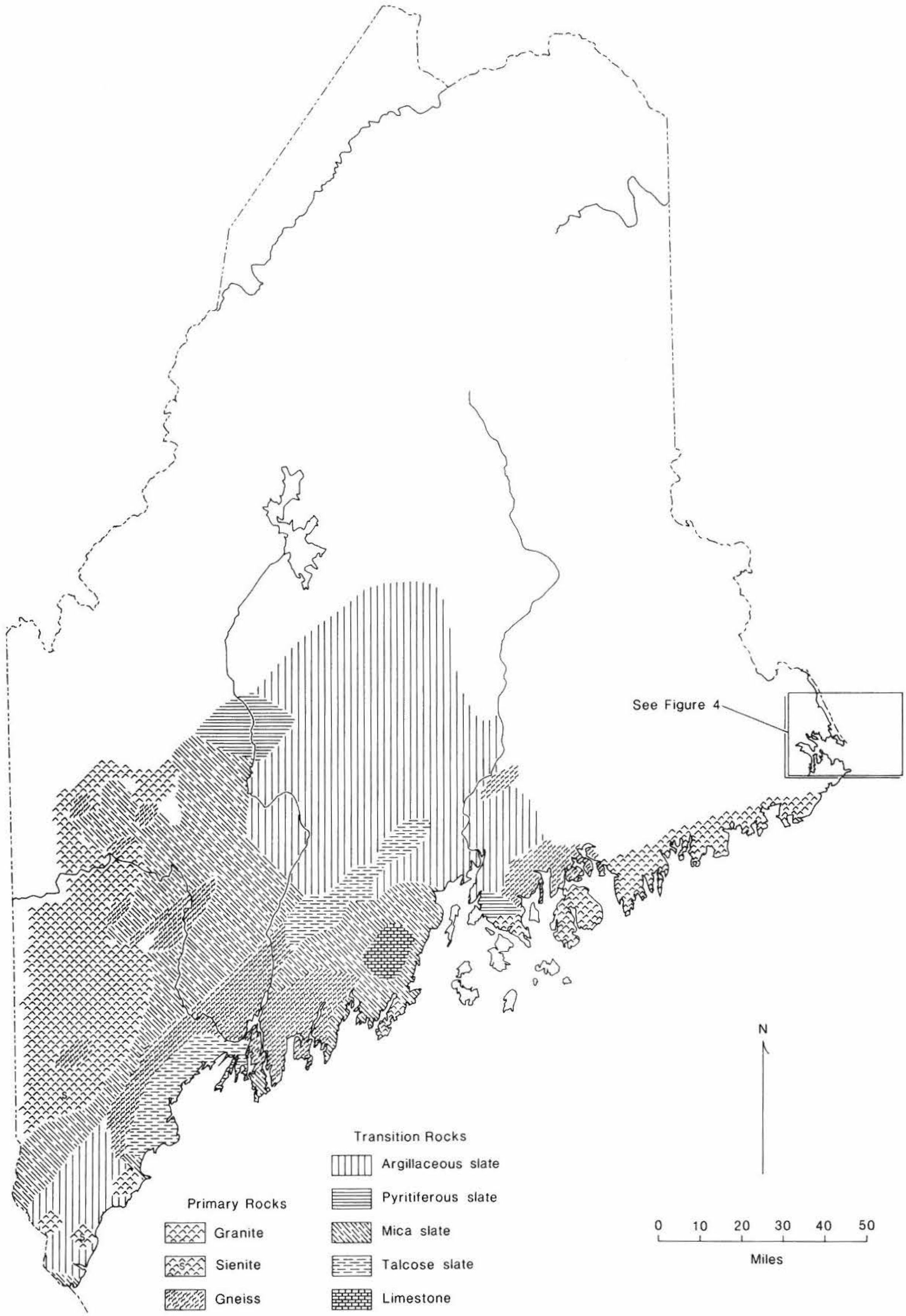

Figure 5. A reconstruction of Jackson's field notes on lithologies in Maine, in map form. Contacts are not indicated. As noted in the text, this figure is a summary and should not be considered the map Jackson would have drawn.

As Aldrich (1981) notes, Jackson's "preoccupation with recording facts led him to report the compass bearing of virtually every scratch on the rocks." The preoccupation begins early in the 1836 field season and continues unabated through the third year of the survey. This apparently inductive activity was accompanied by a strengthening belief that most of the unconsolidated materials found in Maine, as well as the scratches, were caused by a current of water. No observation of an isolated scratch stood alone; each was used to reinforce Jackson's position that

> . . . there is a striking coincidence between the direction of these marks and the diluvial grooves which I have noticed before. Those in Portland run from N. 15° W. to S. 15° or 20° E. and here [in Charleston], in a distant portion of the state, nearly the same direction is observed. We have, however, many more equally good illustrations of this subject. (Jackson, 1838, p. 28)

Ideally, Jackson might have mapped the scratches, but the verbal descriptions of the diluvial phenomena are nearly as graphic as a map. More important, the recording of diluvial scratches allowed Jackson to determine the source directions of erratics and of the parent materials for soils:

> It will be readily conceived, that if solid rocks were moved from their native beds, and carried forward several miles, that the finer particles of soil should have been transported to a still greater distance, so we find that the whole mass of loose materials on the surface has been removed southwardly, and the soil resting upon the surface of the rocks, in place, is rarely, if ever, such as results from the decomposition of those rocks, but was evidently derived from those ledges which occur to the Northward. (Jackson, 1838, p. 149)

Thus, despite the wealth of lime-rich rocks in Thomaston, Jackson finds the soil calcium-poor,

> . . . derived from the decomposition and disintegration of granite, gneiss, and mica rocks which lie to the northward of that town.
>
> This fact accounts for the almost entire absence of carbonate of lime in the soil, and indicates at once to the farmer, that liming is there extensively required. (Jackson, 1839, p. 62)

It would be convenient to interpret Jackson's diluvialism as *heuristic*, that is, as a unifying concept which furthered his investigations but which was held tentatively or understood to have no basis in nature. It would be a mistake, however, to treat Jackson's commitment to diluvialism as a mere heuristic; for better or for worse, the commitment was extensive. Controverting the Hutton-Playfair account of valley excavation, in which streams are understood to have carved the valleys through which they flow, Jackson claimed that the geomorphology of the Kennebec valley area in and around Augusta could be explained through the deluge hypothesis: ". . . anyone who looks upon the general direction of these valleys, will feel satisfied, that they were excavated by a current of water" (Jackson, 1837, p. 84). Moreover, Jackson developed ad hoc explanations for diluvial data where these countered the overall trend. In Phillips, Jackson encountered "several remarkable phenomena":

> First, the occurrence of diluvial markings, which do not coincide with the direction formerly noted, as the general bearing. Secondly, the occurrence of extremely heavy masses of iron ore of foreign origin, and granite rocks also erratic, poised upon the summit of an insulated hill. The questions that naturally arise are, first—how came these scratches on the surface of the ledge? And secondly—why, if they owe their origin to causes I have formerly assigned, do they vary in their course? (Jackson, 1839, p. 28)

The anomaly, though puzzling, did not lead Jackson to question the diluvial explanation. Instead, he adjusted to the data, using the surrounding mountainous topography to provide a distorted sluiceway through which the deluge behaved with corresponding turbulence:

> . . . this apparent anomaly in the direction of the diluvial scratches, is a most striking and wonderful confirmation of the theory which we have enunciated; because the shape of the country, as is evident to any observer, would have caused the precise deflection observed in this case; for Mt. Abraham arrested the current on the north and turned it into Sandy River valley on the west, from which deflection it struck against the Mt. Saddleback range, continued to Mt. Blue, and by Saddleback was reflected, precisely according to the well known laws of physics, towards French's Mountain; and thus the marks coincide with the direction of the two forces. It moreover proves incontestably that the current did not set in from the S.E., for the course would have been at right angles with the present markings. (Jackson, 1839, p. 29)

Here, a map would have been helpful, for Jackson's description is insufficiently graphic to provide a reading of the phenomenon. The problem, alas, is not reconstructible in modern terms. Yet it is an ad hoc explanation to which Jackson became committed in explaining additional anomalous striae (Jackson, 1839, p. 32, 43).

As noted by Aldrich (1981), Jackson observed "the power of ice in moving boulders during the spring thaw on New England rivers, but he did not use the mechanism, in the form of glaciers, to explain the rock gouges, transport, scouring, or moraines which decorate the state's landscape." Additionally, Jackson seems to have rejected Lyell's proposal for transport by icebergs as well as the glacial theory (Merrill, 1904, p. 348), the latter on the grounds that the striations should show a radial distribution in mountainous areas, which they generally do not.

Such an objection seems to be out of proportion to obvious objections to the diluvial theory. Buckland, beginning with his recantation of diluvialism in the Bridgewater Treatise, continued to search for an explanation for the diluvial phenomena and accepted Agassiz's explanation well in advance of Lyell. As Rupke has argued,

> Intellectually the change from a diluvial to a glacial mechanism of boulder emplacement was very small indeed; in the place of [Sir James] Hall's tidal wave came a huge mass of frozen water. (Rupke, 1983, p. 106)

Jackson, however, remained adamantly opposed to the glacial theory, apparently throughout his life. Nevertheless, he made a contribution to understanding the surficial geology of Maine. In part, this is because the difference between the glacial theory and the discredited diluvial theory has been established through inferences from terrains that are currently glaciated. No *direct* empirical evidence exists to decide whether a flood or a sheet of ice existed here at a particular point in time. The theories are, however, clearly incompatible vis-à-vis some aspects of flow mechanisms and drift transport. But in terms of heuristic power, they are virtually equivalent systems when applied to the workaday problems of the state geologist in the 1830's. Greene shows that this distinction is not by any means trivial. Quoting an account by Darwin of a field trip in Wales with Adam Sedgwick, wherein abundant glacial phenomena were completely overlooked by both geologists, Greene states that

> Darwin did observe glacial phenomena in the colloquial sense, but he did not see them as elements joined together by a theory and therefore did not remark upon them. What he lacked to understand them on that first visit was not powers of observation but some concept that would have extracted an organized body of fact from a jumble of stones. (Greene, 1982, p. 59-60)

Jackson lacked no such system. Moreover, in the collection of data, the term "diluvial scratch" may be transposed with "glacial striae" with virtually no loss of meaning, as Aldrich points out (1981, p. 6). And Jackson's contribution to an understanding of the origin of Maine's soils changes not a whit. Characteristically, Jackson did not prepare a map from his diluvial data. In Figure 6 such a map has been constructed from the reports. The difference between the reconstructed map and the present surficial map (Thompson and Borns, 1985) – apart from terminology – is quantitative, not qualitative.

Beyond the question of whether Jackson's interpretation of the striae, gravels and erratics was "right" or "wrong," there emerges another theme: the clear sense that the diluvial geology in Jackson's reports is, in fact, interpretation. Although the diluvial interpretation stands opposed to Jackson's apparently inductive methodology, giving lie to his claim that "nature dictates . . .," Jackson's descriptions of diluvial phenomena go far beyond the purely observational.

In addition to recording and interpreting the diluvium, Jackson also commented upon the appearance of clays throughout the state's coastal plain. At Lubec, in 1836, he found recent "marine shells . . .in regular layers imbedded in the clay" in an excavation for a tidal power canal. Jackson related this phenomenon to the erosion of greenstone trap at some distance from the ocean and asks "Has the level of the sea become depressed or have the rocks been elevated?" Lacking evidence for a regression of sea level, Jackson concluded that the land had emerged "within the recent Zoological period" (Jackson, 1837, p. 19). As was the case for the diluvial interpretation, Jackson's recognition of emergence provided a coherent explanation for the clay deposits in the state. In the second report, he placed the limit of sea level transgression at 100 feet, but increased that limit to 150 feet in the third season.

To call Jackson a "catastrophist" would not be correct. But he clearly saw the geomorphology of Maine as of two types: that of some prior time and that of the contemporary topography. Jackson was no devotee of Lyell, as Aldrich notes (1981, p. 6). A final example of his investigations of unconsolidated materials, however, may serve to provide a perspective on his theoretical commitments. While conducting a reconnaissance through Limerick, Jackson was drawn to a peat bog that had recently been drained, and in which one Ebenezer Adams claimed to have found coal "amid the remains of rotten logs and beaver sticks" (Jackson, 1838, p. 80-81). Jackson did a chemical analysis of the finding, and pronounced it "a true bituminous coal." If it occurred to Jackson that Adams was simply having some fun at the expense of a government geologist, he did not say so. Instead, Jackson wrote that

> The discovery of the recent formation of bituminous coal cuts the gordian knot which geologists and chemists are endeavoring to unravel, and shows that the process is still going on. (Jackson, 1838, p. 81)

This was not the sort of comment a "catastrophist" might be expected to make. As Greene has argued, the catastrophist/uniformitarianist debate was the invention of Charles Lyell – a rhetorical device used by the trained barrister to make a case (Greene, 1982, p. 25-26). Jackson did not systemize his findings; instead, he employed *and expanded upon* a series of heuristics to interpret the landscape of Maine. So long as none of the interpretations contradicted any other, such an approach was both necessary and sufficient.

Economic and agricultural geology

Jackson's overwhelming concern in the survey of Maine was to find economic value in the rocks. Accordingly, much space in the reports was given over to discussions of the granite and limestone quarries in the state, and to determining the economic values of bog irons, peat bogs, and minor veins and deposits of ores throughout the state. Aldrich (1981) has discussed Jackson's contribution to the economic and agricultural geology of Maine, and little more needs to be said. It is noteworthy, however, that Jackson recognized the natural beauty of Maine and recommended encouragement of a tourist industry in places like Moosehead, Blue Hill, and Denmark.

Although he suspected that coal might be found in the eastern part of the state, Jackson was unequivocal about coal speculations elsewhere in Maine. At Small Point, Jackson inspected some coal that had washed up on the shore. This, by chemical analysis, was identical with Orrel coal from England, and Jackson pointed out that

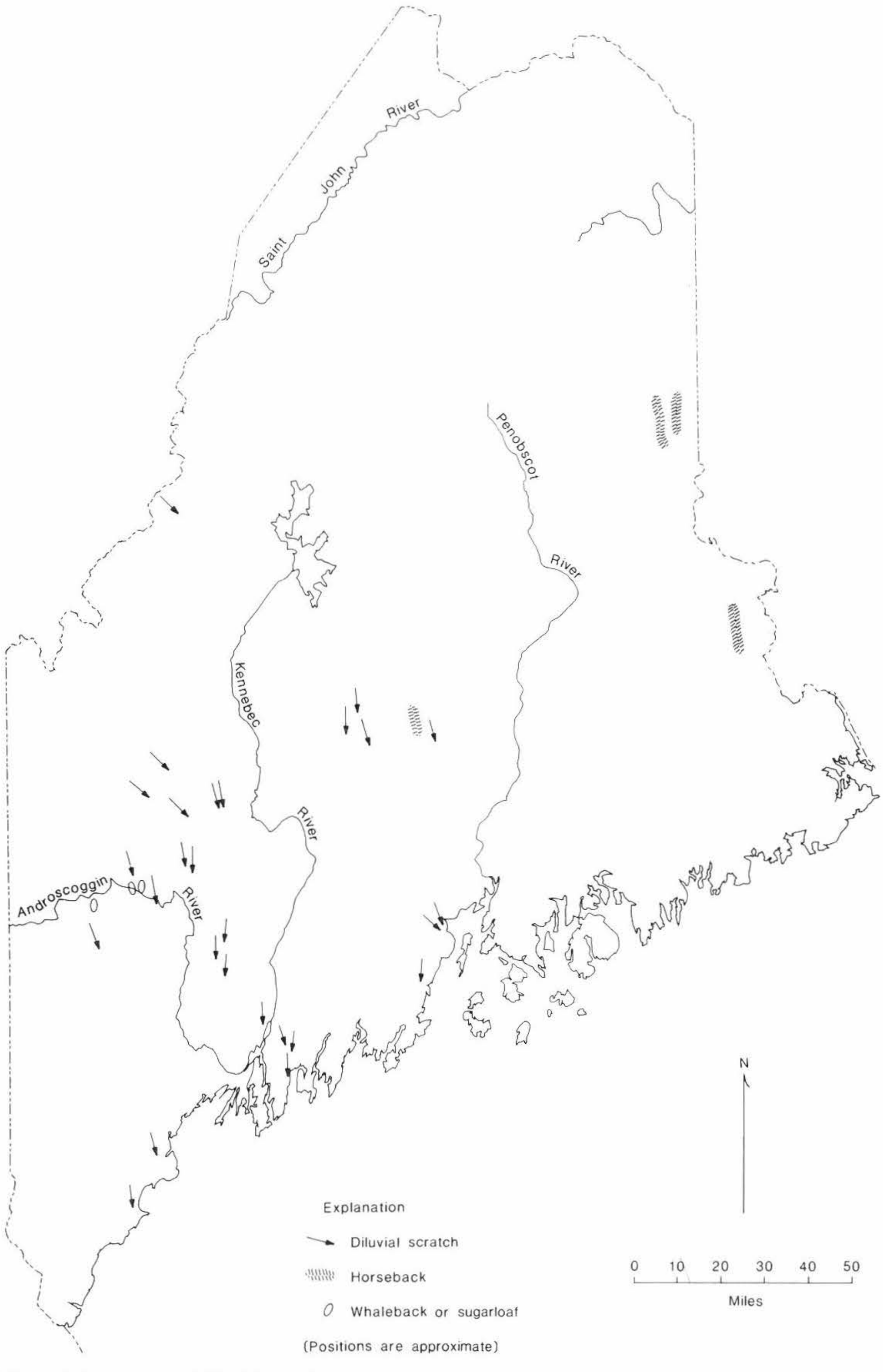

Figure 6. A summary of diluvial scratches, horsebacks, and whalebacks from Jackson's reports, in map form.

> . . . the rocks along this coast were . . . gneiss, a primary rock in which coal is never found, and the beach consists of silicious sand . . . evidently derived from the disintegration of similar rocks. (Jackson, 1837, p. 82)

Often, he would hear of a suspected coal-bearing rock but, on examining the rock, would find tourmaline, black oxide of manganese, or graphite. Jackson debunked these speculations, usually by pointing out simply that primary, or crystalline rock, could contain no coal.

> We are never to look for that combustible lower down in the series than the newer transition, nor above the secondary. Hence the absurdity of searching in granite and mica slate rocks, for beds of coal, and the mistakes arising from the occurrence of lignite in the tertiary clay – both common and fatal errors to those who engage in such absurd enterprises. (Jackson, 1839, p. xiii)

Rocks with a high sulfur content near Castine were thought by many to be a coal indicator:

> It was . . . originally imagined by the English, during the late war, that a coal mine existed in this spot, for as coal frequently contains sulfur, they thought it probable that a rock containing sulfur must necessarily contain coal. Several other persons have since been deceived in a similar manner, and within a few years borings were made for coal. The auger penetrated to the depth of 100 feet, and brought up nothing but pyritiferous slate, as might have been anticipated. . . . Now had this locality been a coal formation, as it certainly is not, there would have been no need of boring, for the strata stand upon their edges, or at an angle of 70° with the horizon, and no person, at all acquainted with the structure of the earth, would ever think of such an operation, for it would not give any information of the kind required. A geological observer can penetrate a thousand feet deep, when such is the position of the rocks, without digging into them at all. It is an open book that is laid before him, and he has only to observe attentively. (Jackson, 1838, p. 47)

This last comment returns us to Jackson's philosophy of science. In his introduction to the second report, Jackson wrote:

> Geology is a science composed almost entirely of facts, and the theories serving to explain them, are but the *rationale* of those facts. Such, at least, is the modern aspect of the science, and the more rigid are we in our deductions, the more imperishable will be the results. Hypotheses may be exploded, theories are subject to continual modifications, according to the light that may be shed upon their subject, but FACTS are in their nature immortal. (Jackson, 1838, p. viii)

Jackson's fact/theory distinction, while not as rigidly held as that of Henry de la Beche (Rudwick, 1985, p. 452-453), was sufficient to blind later readers to the extensiveness of Jackson's theoretical interpretation. Several examples – the diluvial anomaly and the bitumenization of peat – have been cited as examples of this interpretive bent on Jackson's part. For late nineteenth century geologists and for modern readers, these interpretations were and are considered wrong, of course, but they are interpretations nonetheless. Just as important is the reciprocal problem in the fact/theory distinction: the presence of folded strata in Maine is a fact, but one that Jackson did not recognize because he held no comprehensive theory of dynamic processes to organize the data-collecting process here. Perhaps, it may be argued, he ought to have devised such a theory.

EPILOGUE

Jackson's reports were published soon after each field season and totaled, including two reports on the public lands, nearly 850 pages. The citizens of Maine undoubtedly considered this much information on rocks sufficient for reasonable purposes, and ceased funding in spite of Jackson's lobbying efforts in print and in person, an effort that was also taken up in Benjamin Silliman's journal. One major review and two short reviews of the reports on Maine published in the *American Journal of Science*, and almost certainly written by Silliman (1839) himself, call Jackson "able and perspicuous" and "one so thoroughly qualified by study and observation. . . ." The lobbying efforts were, however, to no avail and the first geological survey of Maine ended following publication of the third annual report. No further state-financed work would be carried out in the state until the Hitchcock survey of the 1860's.

Jackson, his reputation bolstered by his work and publications in Maine, went almost immediately to work in Rhode Island and published a single (and final) report for that state in 1840. From there, Jackson moved on to New Hampshire; this survey required four years (Aldrich, 1981, p. 8) but, having learned a lesson in Maine, Jackson withheld his findings for the publication of a "final report" in 1844. Geologic maps accompany both the Rhode Island and New Hampshire reports. Moreover, Jackson made an attempt to interpret the overall geology of New England in his New Hampshire volume (Aldrich, 1981, p. 8-9).

Throughout this period in his life, Jackson belonged to and helped to found several professional societies. Through them and through letters, he sought to increase public education in geology, and seriously proposed that every state survey provide for as many as fifteen duplicate collections of samples to be shipped off to colleges, in addition to a collection for the state government. Merrill records the sad fate of the 1,566 specimens of Jackson's "state cabinet" in Augusta which "were thrown promiscuously into boxes and otherwise in disarray"; most of these were transferred to Colby College in 1888 (Merrill, 1920, p. 132-133).

Following the New Hampshire survey, Jackson enjoyed a three-year hiatus from government geological work. In 1847 and 1848 he worked as a United States geologist in Michigan, but resigned for reasons that remain obscure (Woodworth, 1897, p. 79-81; Merrill, 1904, p. 414-415). With the Michigan survey, Jackson's career as a government geologist came to an end.

Almost simultaneously, a storm of priority disputes began over the discovery of the anesthetic value of ether and the invention of electrical telegraphy; these cannot be dealt with here except to say that the disputes became the focus of Jackson's later life. Accounts of the priority dispute in Woodworth (1897) and Bouvé (1880) give credence to Jackson's claim, but for the modern reader, Jackson appears as a nineteenth-century Robert Hooke, claiming priority for discoveries about which he had published little or nothing.

By 1873, Jackson was overcome by an apparent mental illness which prevented any further work. He died in 1880 at Massachusetts General Hospital. Thomas Bouvé, writing the memorial remarks for the Boston Society of Natural History, said

> The truth is, Dr. Jackson was a man of great genius, and his intuitive perception of scientific truths remarkable; but from some peculiarities hard to comprehend, he often contented himself with enunciating what he recognized as a fact, without striving to substantiate it. (Bouvé, 1880, p. 46)

Woodworth, in a more expansive tone aided by distance in time, had this to say:

> Jackson was a genius. He had the inventive faculty; the habit of incessant investigation; the capacity of getting tangible, fruitful results; and the ability to suggest successful expedients to others. Geologists think of him as a geologist. (Woodworth, 1897, p. 85)

CONCLUSION

C. T. Jackson is not remembered today as a genius. Beginning with Merrill (1904) and continuing through Aldrich (1981), the historical assessment of Jackson's work has ranged from negative to neutral, at best. The present revision in the historical judgment of Jackson might appear at first glance to be an apology. But the apologist need argue only that Jackson fulfilled the political mandate for a "state survey" with notable efficiency, and with an attention to political realities which was uncharacteristic of the early state surveys viewed as a whole. Indeed, the apologist could argue that the meaning of "geological survey" has changed since the 1830's in a way that renders Jackson's work incommensurable with later "surveys." This is not, however, an apology. For to the question: why has Jackson been "assigned a lesser place in the pantheon of earlier investigators of the Northeast"? (Aldrich, 1981, p. 10), there is a reciprocal question: why be concerned about Jackson at all? The latter question is more easily answered than the first. The answer has to do with the revolution in the history of science proposed by Kuhn in *The Structure of Scientific Revolutions* (1970), a revolution that has yet to be carried through. Kuhn's book has been interpreted to suggest that the examination of the scientific process would benefit, indeed may not be possible without, an examination of "normal science." Normal science is, as Rudwick (1985) has put it, "the ordinary business of scientific research [which] is carried on within a shared or collective framework of methodological assumptions, heuristic maxims, routine procedures, observational and experimental standards, criteria of interpretative judgement, and much else besides." It encompasses the kind of discovery that results from empirical field work and experimentation. Simply put, knowing where to look, when to look, and for what to look leads to the kind of discovery that is characteristic of the field geologist in normal scientific practice. Investigating "normal science" as part of the Kuhnian research project has hardly begun.

Jackson, for the period of the state surveys, was doing normal science. The foregoing details of his survey of Maine are but a vignette of the normal science practiced by American geologists in the 1830's. As Daniels has written,

> Whatever the fate of natural-history theories, natural history descriptions are not so likely to be superceded as they are to be elaborated and refined. The history of scientific progress, therefore, has a place for those who wrote the early descriptions. . . . An over-zealous attention to scientific progress has obscured the entire nature of the early nineteenth century scientific community. (Daniels, 1968, p. 32)

Moreover, the various observational practices, assumptions, maxims, procedures, and the like applied by Jackson to the survey of Maine were dictated by a felicitous match of his so-called "Wernerian" commitments and mineralogical biases to the problems of reconnoitering the geology of Maine. In contrast, the Lyellian metatheory of the 1830's had little application to the geology of Maine, and a first survey by an accomplished biostratigrapher might well have produced far less of importance than Jackson's, had it been possible at all.

That Jackson was not a Lyell, a Hutton, a Murchison, or even a Werner is obvious enough. But his "lesser place in the pantheon" has not been assigned relative to these figures in the history of geology. Rather, it is relative to James Hall and the Rogers brothers who developed metatheories to explain what they found in New York and Ohio, Pennsylvania, and New Jersey. But the geology of Maine is daunting, both in terms of complexity and of accessibility, as any field geologist who has worked here will attest. The development and application of metatheories must be made with caution. The structure of the geology of Maine is exceedingly complicated, and correlation is difficult and as yet incomplete. As a type area for the phenomenon of post-glacial coastal submergence, Maine is unique in the United States. Accordingly, a survey-for-survey comparison of the early state geologists is misleading, for these geologists did not work on the same problems or under comparable constraints.

Jackson was a competent field geologist, accomplished at fine-scale interpretation, and politically attuned if not astute. But in terms of modern geology, he was "wrong" much of the time. If the ultimate goal of the history of geology is an object lesson – and who is to say that it is not? – there is object lesson enough in the story of C. T. Jackson.

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